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Meditation and Somatic Arousal: A Comment on Holmes's Review

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Holmes's (January 1984) conclusion that there is no evidence to prove that meditation lowers somatic arousal is based primarily on his systematic detailing of the methodological faults in what was once considered conclusive research. But is this type of critical analysis a constructive epistemological strategy? In a science such as psychology, where methodological and conceptual dilemmas abound, one may find flaws in any experiment. Other reviews of the same studies have focused on their strengths and on their contribution to our understanding of meditation. These reviews also provide a more unified and parsimonious interpretation of the research findings, namely, that meditation can lower somatic arousal.

Other issues come to mind upon reading Holmes's article. Certain psychophysiological events, such as the law of initial values, may indeed confound the evidence suggesting that meditators experience lower arousal than nonmeditators, but we should also consider other important psychophysiological phenomena that may complicate or obscure the effect of meditation, such as autonomic response specificity and directional fractionation (Lacey, 1959, 1967). Holmes also noted that there is no consistent evidence *across* psychophysiological measures that meditation reduces somatic activity. Yet, it is well known in psychophysiological research that there is rarely any unified or global pattern of arousal (Lacey, 1959, 1967; Obrist, 1976). We could, as Holmes suggested, invalidate experiments that demonstrated the hypothesized effects of meditation by invoking the specter of unknown, unpublished research that never obtained significant results, but the same ghost could be summoned to haunt any classic experiment in psychology. Should we automatically disregard case studies and research lacking appropriate control groups, even when they demonstrate the remarkable physiological feats of expert meditators? On an even more fundamental level, what do we mean by "meditation" as compared to simply "resting"? A more in-depth conceptual and methodological analysis of these independent variables is needed. Holmes seemed to focus on transcendental meditation as an operational definition of meditation, but this is only one of many passive meditation techniques. Finally, if it is indeed true that meditation does not affect somatic activity, let us be careful to avoid conclusions that its effectiveness in other realms must therefore be restricted—a conclusion that would be based on an implicit physiological reductionism. Is it even meaningful to draw any conclusions about the effect of meditation on somatic arousal without relating this to the psychological aspects of arousal and consciousness?

Meditation is an ancient therapeutic technique that has been studied and practiced by many individuals of far-reaching intellect and insight. It has endured the rise and fall of civilizations, and predates both science and psychology by many centuries. As scientists who sometimes do not bother climbing onto the shoulders of our predecessors, let us carefully examine any conclusions about its ineffectiveness.

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Meditation and Somatic Arousal Reduction

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Research examining the effectiveness of meditation in reducing somatic arousal has been used by evangelists to justify the application of meditation in a variety of therapeutic settings. Initial claims of the discovery of a major "hypometabolic physiologic state" (Wallace, Benson, & Wilson, 1971) have been tempered by the recognition that "there are decreases in arousal during meditation which do not appear to be significantly different from decreases in arousal observed during other relaxing activities" (West, 1979a). Holmes (January 1984), in a careful review of the evidence, concluded that "within the existing research there is no evidence that meditation is more effective for re-

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